Review of the paper entitled "Interactive ocean bathymetry and coastlines for simulating the last deglaciation with the Max Planck Institute Earth System Model (MPI-ESM-v1.2)" by Virna Loana Meccia and Uwe Mikolajewicz.

This paper has been long awaited by the community working on the last deglaciation from LGM to present day. But, in fact, this methodology could also be interesting for simulations for future deglaciations of Greenland and West Antarctica in the next century.

Indeed in the framework of PMIP4 deglaciation project (Ivanovic 2016) in which models intend to provide transient simulations from LGM to PD, such tool is absolutely needed.

The authors aim to use the MPI ESM to produce deglaciation transient runs. They cope with a long lasting issue and resolve it: how to modify boundary conditions that account for sea level rise during the deglaciation and modify the topography (bathymetry and coastal lines) all along this process using algorithms that avoid manual and more or less subjective corrections. They describe the algorithms they used for adaptation of the ocean model MPIO at low resolution used in the PMIP4 exercise with boundary conditions evolving every 10 year.

The paper is well written and its structure is clear. The detailed description of strategy target and problems is convincing.

My major comments are the following:

1 the paper is perfectly suited for GMD. Nevertheless the authors never tackle the effect of their boundary condition changes on deglaciation. Therefore I suggest that they address this question at least concerning two important points

- Discussing the added value of this study compared to previous simulations where the bathymetry was not changed to better emphasize what may be the interest of this study beyond the technical challenges.
- The authors should also emphasize the potential limitations of this method in terms of simulating abrupt events during deglaciation due to many linear processes they used, both in time and space. I perfectly understand smoothing procedures the authors described to avoid crash of the model, but during deglaciation many non linear changes occurred for instance MPW and more generally acceleration of melting rates described for instance in C. Waelbroeck et al., Quaternary Science Reviews 21, 295-305, 2002, for the last 30k. Therefore the authors should discuss in more details what is the compromise between avoiding crash and capturing real non linear events.

2. The authors should also clarify the part of the paper that may be directly useful for the PMIP4 deglaciation community and those that have been developed specifically for MPI ESM.
Whereas this paper is worth to be published in GMD, I have also minor comments that it would be important the authors answer to before publication.

Minor comments:

Abstract:

A1: What do the authors mean by conservation of mass and tracers at regional scale. It is a bit misleading in the abstract. I think the authors have in mind to keep regional conservation when changing spatial resolution. They should clarify this issue.

A2 The authors, first tackle a very general problem: the bathymetry adaptation when simulating the last deglaciation. How far the algorithm developed here, beyond grid specificity can be easily adapted to other models. A sentence in the abstract should clarify this point.

Introduction

I1 The first sentence is very general and partially untrue because of some aspects of the unprecedented speed of ongoing climate change. The authors should remove or modify this sentence.

I2 The authors should mention that major uncertainties remain on reconstruction of Antarctica at LGM. Indeed, NH ice sheet reconstructions are better constrained, whereas Antarctica ice sheet reconstruction has often been an adjustable parameter. Therefore, the authors should mention Antarctica reconstruction uncertainties at LGM both from data and models (G. Philippon, Earth and Planetary Science Letters 248 (2006) 750.)

I3 The authors should also mention that there have been already many successful publications on glacial-interglacial simulations cycles from EMIC (A. Ganopolski et al., Nature 529, pages 200–203 (2016)) and from GCM (A. Abe-Ouchi et al., Nature 500, (2013)190. Moreover, the authors should better emphasize what in this context would be the added value of accounting for sea level rise.

I4 Superimposed to the vertical resolution of MPIO, an important issue to be discussed is the choice of the initial horizontal resolution.

Methodology:

M1 It is not clear for me that accounting for only two big lakes (Caspian and Black Sea), the authors can capture abrupt climate changes occurring during deglaciation, as for instance the 8.2 ka event. Moreover, the evolution of Caspian and Black Sea associated to Eurasian ice-sheet melting and large modification of the catchment is not easy to be reconstructed and depicted. The authors should clarify more explicitly what is the limit of their method. Specifically, they should explain how they cope with river run-off and changes in catchment areas during deglaciation for these two epicontinental seas. These issues have been shown to have drastic consequences on atmosphere and ocean circulation (see for example R. Alkama et al., GRL 33 (21) 2006, R. Alkama et al., 2008, Climate Dynamics. 30 and M. Wary et al, J. Quaternary Sci. 32, 908–922, 2017).

M2 At the end of paragraph 2.3, in the spatial smoothing procedure for SSH, there are also changes in water mass reorganization that lead to spatial variations of the sea level rise during melting as shown for instance in Mitrovica (Nature 2001,…). Is this effect accounted for? If not, the authors should clarify the possible impact of this process.

Results:

R1: Whereas this paper is submitted for publication in GMD and devoted to technical and model development aspects, it is difficult to consider the validity of the process only analyzing the stability of the response without any information on the potential climate effect. Indeed, accounting for bathymetry with time steps of 10 years should allow the authors to capture the complex pattern of the deglaciation periods. Nevertheless, due to linear smoothing in time and space, it is unclear to me whether they really may capture abrupt events. This limitation should be discussed in more details.

R2: Superimposed to ice sheet melting, a major component of the SLR is the ocean thermal expansion during deglaciation. Therefore it should produce a difference between SLR and cumulative fresh water input. In fig. 8, I suggest to plot, superimposed to the black and red curves, the component relative to the changes of the ocean volume associated with the thermal expansion during deglaciation.

R3: is the model accounting for a possible ice shelf at the beginning of the deglaciation in the northern hemisphere?

Remarks:

RM1 As the impact on climate due to change in bathymetry is not described in this paper, we can still have in mind many questions concerning the limits of this tool, when applied to non linear processes as those occurring during deglaciation. Indeed, the deglaciation is far to be a linear process. Major abrupt events (MWP and HE) occurred that are associated with large increase of fresh water inputs. It would be interesting that the authors discuss these potential limitations.

Final comment:

This study is interesting and novel. Moreover, it corresponds to an awaited development to better simulate the last transient deglaciation. Therefore when the authors will have answered the questions raised above, the manuscript will be worth to be published.